THE VETERINARY RECORD

A Weekly Journal for the Profession.

No. 354. SATURDAY, APRIL 20, 1895. Vol. VII.

Selection by Nationality.

There are various methods of selection adopted to obtain good men for important offices. The Chinese have a very simple one. They adopt a competitive written examination on the classic authors of China —especially the books of Confucius. From the best men thus selected appointments under government are made—some to the Navy, some to the Army, some to the Judicature and some to the Engineering department. It is believed in Europe that the Japanese have exposed the weakness of this method of selection. There are other methods. In Turkey the highest bidder often gets office. In this country it is believed that a friend at Court is useful; and at the Board of Agriculture, service in the Navy, or Secretaryship to a Cabinet Minister seems to be almost indispensable. Probably the supporters of every method could supply reasons to show that their object was to obtain the services of the best man. The Royal College of Veterinary Surgeons leaves to its Council the selection of examiners, and has carefully framed rules to assist in obtaining the best men. No member of Council may offer himself for election. Every candidate must be nominated some time before the selection is made. The election is conducted by ballot. The Council itself is a representative body elected by ballot and includes practitioners from England, Scotland, and Ireland.

During the last 30 years the Board of Examiners has been chosen irrespective of height, complexion, or nationality. No one has ever yet inquired the avoirdupois weight of an examiner, or insisted upon knowing if he were Scotch or Irish. Such narrow ignorance has been left till this year of grace, and a person signing himself "An Irish Practitioner" has written to a local paper carping at the recent selection of examiners. His letter will be found on our last page. He appears to think that the Council do wrong to select men irrespective of their personal or national claims. His notion is that the Board of Examiners should be parcelled out between the three kingdoms, and that because on the present Board no Irish resident is elected the Council have acted upon some political bias—on the principle, in fact, that "No Irish need apply."

An accusation of this kind, if really made by a member of the profession, is as cowardly as untrue. Possibly however we have mistaken the writer's spirit. His letter may have been dictated not by malevolence but by ignorance, and his statement that "There are plenty of men in the veterinary profession thoroughly well qualified to act as examiners on pathology" is strong evidence in this direction. It is only partially true to say that the College elect "medical" men as examiners in that subject. The men elected may have a medical degree but they are not medical practitioners. They are experts on that subject—men who confine themselves to that subject. Outside of the hospitals and medical schools we should no more find "medical" practitioners fit to examine on pathology than we now find "veterinary" practitioners. The three gentlemen from Ireland who were candidates have our sincere sympathy for having been referred to by their misguided champion. They would be the last to support him.

The Board of Agriculture.

The following remarks appear on the editorial page of *The North British Agriculturist* this week.

THE 'ANIMALS' DIVISION.

"The stockowners of Great Britain may well join with the members of the veterinary profession in raising an indignant protest against the way in which the profession is being slighted and belittled by the Board of Agriculture. One of the great arguments urged in favour of the establishment of a Board of Agriculture was that this Board, by means of its splendidly-staffed and admirably-equipped veterinary department, would be able to act in the most prompt and effective manner in the suppression of all forms of contagious disease. How far this promise has been implemented may be judged by the imbecility and gross mismanagement which have characterised the efforts made by the Board for the stamping out of swine fever during the last eighteen months. The cause of this gross blundering has now been fully exposed, and the veterinary department must be declared free of all blame in the matter, seeing that it was abolished at the very time this gross blundering was commenced.

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It is difficult to understand how the Board of Agriculture are always so ready to provide berths with 'good fat salaries' for naval and military officers who have utterly failed to distinguish themselves in either the army or the navy, and who 'hardly know a horse from a cow,' as Mr. Chaplin once said. Formerly, too, the Board of Agriculture had three veterinary experts at their service, namely, Professor Brown, Mr. Cope, and Mr. Duguid. But now when the Board are being hopelessly baffled in their imbecile attempts to stamp out swine fever, and when the much more serious task of stamping out tuberculosis is waiting to be grappled with, the veterinary officers in the service of the Board have been reduced to two, namely, Mr. Cope and Mr. Duguid. Altogether this gross blundering and jobbery of the Board of Agriculture in abolishing the veterinary department of the Board, and substituting in place thereof an 'Animals' department, headed by an undistinguished ex-military officer, combined with this reduction in the effective veterinary staff of the Board, is not only a gross slight to the veterinary profession, but is a shameless trifling with the interests of stockowners and the public generally. The Council of the Royal College of Veterinary Surgeons have drawn up a dignified protest against the policy of the Board in making these changes, and as soon as Parliament re-assembles after the Easter recess Mr. Gardner will be ruthlessly impeached for his wretched blundering in this matter."

CASES AND ARTICLES.

ACUTE PARENCHYMATOUS NEPHRITIS,

COMPLICATED WITH STRANGULATION OF THE SMALL INTESTINE BY THE MESENTERY.

By E. WALLIS HOARE, F.R.C.V.S., Cork.

Subject. A bay hunter, rising five years old.

Previous History. Had been laid up with a wound on the off hind fetlock for about five weeks, and getting only walking exercise lately. He was a very long-backed horse and never put on much condition, but up to the present time appeared healthy.

On March 5th he was noticed dull and off his feed, with slight cough and visible mucous membranes injected, temperature slightly elevated. He was treated with simple febrifuges and appeared to progress favourably until March 7th.

Clinical History. On the above date he seemed very dull and inclined to rest his head on the manger. At 4.30 p.m. after eating portion of a bran mash he appeared uneasy, lay down, breathed heavily, and kept constantly pressing his head backwards and endeavouring to rest the head on the poll. He rose occasionally, pawed with each foreleg alternately, and appeared in slow continuous pain. There was apparent loss of power in the hind extremities when attempting to rise, and he occasionally wandered about the stall. The bowels and kidneys acted naturally. He had received occasional doses of opiates during the night, but no improvement was noticed and he lay in the position described all the day of March 8th, having refused all food.

At 7 p.m. on this date I was called in consultation and found the animal stretched at full length in the stall, with the head extended as far backward as possible, and constantly being worked up and down with attempts to rest it on the poll, the nose being turned upwards. The animal appeared dull and sleepy, and occasionally moved the fore and hind limbs in a semi-unconscious manner. The temperature was 103°, pulse full and strong, respirations accelerated, visible mucous membranes deeply injected, pupils dilated, sclerotic coat injected, slight sweating over surface of body. Fæces and urine had just been passed before my examination of the patient. After some difficulty the animal was made to rise, he appeared to have partial loss of control over the hind extremities. The respirations were greatly accelerated, and auscultation revealed the presence of slight crepitation in the left lung, and slight dullness on percussion; right lung normal. He looked towards both flanks frequently with his head turned in a peculiar manner sideways. He pawed frequently with alternate limbs. While standing he kept the head depressed for long intervals, and moved the jaws as if endeavouring to chew imaginary food, occasionally making movements resembling those of wind-sucking. He seemed as if in dull pain and yet unable to express it by movements. The tail was kept erect from the body and occasionally there were quivering movements to be observed in this organ.

I gave no diagnosis but prescribed Morphinæ Mur. gr.x, Chloral Hydras ʒj, Aquæ Oj, to be repeated in two hours if the animal did not appear relieved, also Quiniæ Sulph. ʒj, Liq. Ammoniæ Acet ʒij, Spts. Eth. Nit. ʒj, every four hours in water. To be allowed oatmeal gruel, linseed tea, etc. *ad. lib.*

March 9th. Visited at 9 a.m., found patient standing and pawing at times in a vigorous manner. Had two doses of Morphine and Chloral during the night, which gave temporary ease. The head was kept depressed, but frequently he turned the nose as close as possible to the right flank. Pressure over this region caused pain, and immediately manipulation was commenced the horse turned his head and kept the nose directed to the part. He was inclined at intervals to press the hind quarters against the door of the stall, and when he lay down it was usually on the left side, with the head directed towards the manger. The tail was still kept erect, the respirations were far easier than the previous night, temperature 102, pulse quicker but inclined to be weaker than before, visible mucous membranes deeply injected. There was total loss of appetite, and the bowels had not acted since the previous day. The urine was passed little and often and the animal stretched out in an unnatural manner while doing so, the penis occasionally hangs pendulous. The animal wandered about the stall in a listless manner, and paws vigorously at times in the corners with alternate feet, the symptoms of pain being of an intermittent character. At times the head is turned to the right flank in a mournful manner and kept there for a long period. Examination per rectum revealed the right kidney very much enlarged and tender on pressure, very little ingesta in colon. Manipulation all over this region was violently resisted by the animal. The catheter was passed after some difficulty, but the bladder was empty and evidently there was very little urine being secreted. An examination of a sample of the urine collected yieded negative results.

Prescribed ʒj. of Chloral Hydrate in solution, this to be repeated if pain was present; also small doses of Ext. Hyoscyamus with Liq. Ammon. Acet and Quinine three times daily. As there was total loss of appetite, it was necessary to administer eggs and milk three times a day. Cold linseed tea was allowed freely, and drank occasionally by the patient. A large linseed poultice was applied to the loins and flanks.

March 10th, 10 a.m. Patient seems much better. Pulse improved, temperature 102. Had passed a small amount of fæces during the night; intestinal murmurs distinct. The urine was passed in small amounts frequently but without much straining, the penis was occasionally pendulous, and the tail still kept erect from the body. The animal still pawed in a listless manner in the corners of his box, and at intervals looked towards the right flank, at other times he kept the head depressed towards the floor. He was frequently found with the hind quarters firmly pressed against the door of the stall. Enemas were administered, also similar medicinal treatment to yesterday. The animal partook of cold linseed

tea sparingly but refused all other food, so it was necessary to forcibly feed him with eggs and milk.

March 11th, 9 a.m. The patient was greatly improved in appearance. Eye much brighter and pulse firmer and stronger. Had passed small amount of fæces during the night. Urine passed frequently in very small amounts, rather thick and oily in appearance. Ascertained that during the night the animal had pawed at intervals. Took small amount of bran and linseed mash, but seemed anxious for hay, and would eat bedding if allowed. The animal now prefers the standing posture and does not lie down. There was still tenderness on pressure over the right flank, but the patient seldom looks towards it. The animal walked very stiffly in the hind quarters when brought out of the stall.

Prescribed small doses of Tr. digitalis along with the other medicines. Gave enema which was not retained.

March 12th. At 8.30 a.m. the patient had a serious relapse. He lay down and commenced to struggle with both fore and hind limbs, and kept pushing back his head in a similar manner to his first attack. He was made to rise and exhibited great loss of power in the hind extremities. Temperature 101, pulse quick and irregular, respirations accelerated, sweated freely over both flanks, had passed small amount of fæces. Urine light in colour, thick, and passed in small quantities. Looked persistently to the right flank, which was very tender on manipulation. Anxious expression of countenance, eyes had a sunken appearance, visible mucous membranes deeply injected. The animal was removed to a more spacious stall, walking with extreme difficulty and requiring support to prevent him from falling. On arriving at the stall which was across the yard, he lay down and gave a few rolls in a listless manner, still preserving the peculiar attitude of the head and neck as previously described.

Prescribed small doses of Tr. Digitalis, and Hyoscyamus, with Potass. Acet., and if the pain became severe chloral was to be given. Eggs, milk, and stimulants in small amounts were also given.

March 13th. Animal had intermittent fit of struggling at night, with periods of apparent ease. There were tremors of the superficial muscles, and occasionally the head was pushed in a backward direction. No secretion of urine apparent, and the animal was unable to rise and died rather quietly in a semi-comatose condition about 12 noon. The examination of the urine on March 12th showed the presence of albumen in large amount, with a small quantity of sugar. I regret that no microscopic examination was made, as I was very much pressed for time.

Post-mortem examination. Body very emaciated, and on opening the abdominal cavity there was a very strong odour of urine from the interior. I examined the right flank very carefully and was surprised to find the ileum, in almost its whole course, strangulated in the mesentery in a complicated manner. It was almost black in color, and great difficulty was experienced in removing it from this position. The mesentery which was almost black in colour seemed firmly entangled around the intestine. On opening the intestine, the interior was acutely inflamed, and contained fluid of a dark red colour. The large intestines contained only a small amount of ingesta. The stomach contained a moderate amount of fluid ingesta, the villous mucous membrane was inflamed in patches and thickened. Here and there were to be found what appeared to be minute ulcerations in the mucous membrane. The right kidney was greatly enlarged, pale in color, and on being removed pus in large amount issued from the ureter. The capsule was not adherent and the organ felt of a doughy consistence to the touch. It weighed 3lb, 1 oz. The left kidney was also enlarged, but of a different appearance to the touch and on section to the right. It weighed 3lb. The left lung was congested; no changes were detected in the other organs. The kidneys were kindly examined microscopically for me at the Pathological Laboratory, Queen's College, Cork, and the result proved the pathological condition to be Acute Parenchymatous Nephritis.

Remarks. I was induced to give the full clinical history of this case, as it is an admirable illustration of the variety of symptoms which may be exhibited in cases of the kind, and the difficulty of forming a correct diagnosis. Evidently the intestinal complication could not have been in existence before the 12th, as on the 11th the animal seemed as if he was likely to recover, and until the 12th no symptoms of acute pain were exhibited.

At one time in the course of the case I was inclined to a suspicion that an intestinal calculus was the cause of the trouble, before the urine gave evidence of at least one pathological condition. I shall be greatly pleased to hear criticisms of the case from practitioners.

VETERINARY SOCIETIES

THE CENTRAL VETERINARY MEDICAL SOCIETY.

The usual monthly meeting of the Society was held on Thursday night the 4th inst., at the Royal College of Veterinary Surgeons, 10 Red Lion Square, Bloomsbury, Mr. S. Villar, the President, occupying the chair. The other Fellows present were:—Messrs. A. L. Butters, Vet.-Capt. E. R. C. Butler, A.V.D., Joshua K. Bruce, Alfred Broad, H. W. Caton, Jno. A. W. Dollar, Henry Edgar, Henry Gray, Prof. F. Hobday, W. Hunting, G. C. Lowe, C. Lowe, Profs. J. Macqueen, J. McFadyean, F. C. Mahon, *Hon. Sec.*, J. H. Manton, W. J. Perryman, Professor J. Penberthy, R. Porch, Charles J. Reakes, F. H. Ridler, A. Rogerson, H. G. Rogers, J. Rowe, *Hon. Treas.*, F. G. Samson, Hugh Singleton, J. E. Tooth, F. Wragg, J. Woodger. *Visitors*: Messrs. A. Bury, F. Dowell, G. E. Cawford, C. Dimmick, Morgan Evans, V. Gazi, Osborne J. Hills, F. King Shaw, G. H. Kitchin, H. M. Lenox Conygham, F. B. Lowe, E. W. Parks, F. Richardson.

On the motion of Mr. H. W. Caton, seconded by Mr. F. G. Samson, the minutes of the preceding meeting were taken as read and were confirmed.

Absentees.

The Hon. Secretary (Mr. F. C. Mahon) announced the receipt of communications intimating the writers' inability to attend from Mr. B. Freer, of Uppingham, Mr. J. Roalfe Cox, Mr. A Prudames, Mr. J. S. Hurndall, and S. H. Slocock. He also read a letter from Mr. W. Perryman, 50 South Lambeth Road, S.W., thanking the Society for having elected him a member.

New Members.

A ballot resulted in the unanimous election of the following gentlemen: Messrs. Ridler, M.R.C.V.S., Silver Street, Notting Hill, W.; John Baxter, Horse Department, G.N.R., King's Cross; E. M. Jarvis, South Kensington, S.W.; Harold Sessions, M.R.C.V.S., Dyke Road, Brighton.

"GLANDERS."

Resumed Discussion on Prof. McFadyean's Paper.

Prof. Macqueen said: Mr. President and gentlemen, unfortunately I did not take the opportunity of making notes while Professor McFadyean was delivering his address, and I hardly think that the summary presented to each of us (a printed summary distributed among the members) gives a fair abstract, or sufficient abstract—for my purpose at least—of the address to which we had the pleasure of listening. But, in the remarks that I wish to make, I shall have to confine myself to the statements made in the summary; and that, perhaps, will be quite sufficient to enable me to comply with the rules of the Society, the precedent of discussion, and the discipline of debate. The first point to which I should like to direct your attention is the history of glanders—not so much the history of glanders in other countries but the progress of the disease in this country. I should like to know whether glanders is on the increase, whether it has increased within recent years, or whether it is gradually disappearing. I can only obtain returns up to 1890 and apparently the disease was on the increase up to that year. That was, of course, before the days of mallein. The returns since 1873 have dealt with farcy as well as glanders. Until 1873 farcy was not recognised as a contagious disease so that, in estimating the number of cases, one requires to add the farcy cases to the glanders cases, and the numbers work out in this way: In the sixteen years—that is, from 1874 to 1890—there were 14,506 fresh outbreaks of farcy and glanders, and 22,629 horses attacked. The largest number of horses attacked in any one year was 1346 in 1880. Coming to 1890, I find the number of horses attacked in that year is 1808. That includes farcy and glanders. The fresh outbreaks in 1890 are 951. Now, perhaps, Prof. McFadyean can inform us if the number of horses attacked and the number of outbreaks have increased or diminished since 1890. I notice that the essayist refers to the early opinion regarding the disease being contagious. There is no doubt that glanders was considered a contagious disease in very early times. In addition to the observers or authorities mentioned by the professor, I find some five or six others and they all apparently strongly believed in the contagious nature of glanders. There are, besides, Solleysel and Bourgelat, Gohier, Volpi of Milan, Delwart, Vitet and Dutz all of whom maintained that the disease was contagious. Our own countryman, White, in 1837 strongly maintained the contagious nature of glanders. Lafosse was doubtful as to the contagious nature of glanders, maintaining that it was sometimes contagious while at other times it was not. Coleman, whose teaching is no doubt responsible for a good deal of error regarding glanders, did not believe in the contagious nature of the disease. Then there is Percival to whose books I might refer you for, perhaps, the best history—the best account—of glanders that has been written up to the present time. I do not refer to recent researches in pathology but to the history of the disease, which is detailed at considerable length. In one volume there are no fewer than 200 pages devoted entirely to the history and contagion of glanders and farcy. As to the cause of glanders, I believe it is no longer disputed, but there are probably some contributing causes which are worth remembering—contributing causes that are often summed up in the word "susceptibility." I should like to hear the essayist's opinion regarding "susceptibility" in relation to glanders; whether he believes excessive work or insufficient food, or food of a poor quality, or defective stabling is a contributory cause? I am very much inclined to think that a great many cases of glanders originate in dark stables where defective drainage and ventilation are well marked, where susceptibility is almost beyond question. Then the essayist says that the glanders bacillus may reach the blood, and that "It was doubtful whether it could be detected in the secretions or excretions; and, although it doubtless at times reached the blood current, there were reasons to believe that it does not long retain its vitality there."

Prof. McFadyean: I do not think I have said that, Prof. Macqueen.

Prof. Macqueen: I am quoting the last paragragh in the summary before the paragraph headed "mode of infection."

Prof. McFadyean: I said that it (the bacillus) did not long remain there (in the blood current.)

Professor Macqueen (continuing): I wish simply to recall to your mind some experiments performed by Cadéac and Malet on the infectivity of the blood. They succeeded sixteen times in transmitting glanders by inoculations of the blood of glandered horses, and they failed thirty-five times. Then the next point in the summary is that the detection of the bacillus in secretions or excretions, is doubtful. Now, this is rather important, and I hope you will excuse me if I try to draw some attention to it. If the bacillus is doubtful, if it cannot be detected in the secretions and excretions of a glandered horse, how does infection occur in cases of occult glanders by which I mean those cases of occult or latent glanders where there is no discharge, no submaxillary swelling and no external symptoms; where there is nothing to indicate the presence of glanders until the horse is subjected to the mallein test, and a post-mortem examination reveals tubercles in the lungs. My point is this: Is a horse with only tubercles in the lungs a dangerous animal in a stable? I want to know what Professor McFadyean thinks; whether he thinks that such a horse can communicate the disease to another horse. Another question on the point I should like to put to the Professor. At what stage of the disease do the lung lesions become infective? and in order to simplify the question a little I should add that I mean at what stage of the lesion is it capable of giving off the bacillus or that infective material which is exhaled or passed out by the glandered horse, and how long may an animal affected with occult glanders continue in contact with a healthy one without danger to the healthy. If a horse with no external symptoms of glanders is not a dangerous horse in a stable otherwise healthy, then it becomes a question whether occult glanders is quite of that importance that we have all along thought. There must be many horses which do not show any symptoms—neither nasal discharge nor ulcer nor submaxillary swelling—and which yet contain glanders tubercles in their lungs; and I want to know if such horses are capable of infecting others in the usual way. The vitality of the virus is another question as to which I desire information. I have no recollection of Professor McFadyean touching the point in his address, but my calling his

attention to the omission may be forgiven if he will complete his statement and tell us about the vitality of the virus, because it has rather an important bearing upon the suppressive measures to be taken after an outbreak.

The next question is the radius of infection. I should like to know how far off a healthy horse may stand harmless from an infected one; whether a healthy horse, before it can be infected, must come into actual contact with or in close proximity to an infected one, and if so, within how many yards. What is the infective radius of glanders? If I understand the Professor aright, he said that glanders bacillus or its product is not inhaled as a gas, and that it is only after the discharge is dry and volatilized that it enters and is diffused in the atmosphere of the stable. Then, according, I will say to his learning, it is inhaled by the sound animal. I want to know whether there is any distinction as to radius. Next I want to know the condition of the infecting material—its condition at the time of infection. Does it pass into the horse in the form of an impalpable powder, in the form of vapour, or as the bacillus itself? Regarding the mode of infection I dare say everyone is agreed that wounds may be infected—that infection may take place through the skin. But Professor McFadyean denies that infection can takes place through the intact skin, and asserts that infection takes place through the respiratory tract and only occasionally through the alimentary canal. The essayist will doubtless remember some experiments performed by Babes to prove the possibility of infection through the intact skin and he succeeded in proving—to his own satisfaction at all events—that infection took place through the hair follicles.

Professor McFadyean: Was that glanders?

Professor Macqueen: Yes.

Professor McFadyean: In the horse?

Professor Macqueen: I think so. I have little to say with regard to the mode of infection which is called respiratory, beyond remarking that Renault for twenty years experimented as to the mode of infection in glanders long before Cadéac and Malet, and there is no doubt that Renault only succeeded six times out of many trials. But unfortunately the details of Renault's experiments are amissing, so that although his experiments are often mentioned, yet there is no very satisfactory account available as to how they were carried out. I notice, however, in quite a recent discussion that Chauveau maintains, I believe on very good authority, that Renault's experiments were conducted with very great care and that they never proved to his (Renault's) own satisfaction that glanders was very often transmitted by the respiratory tract. Cadéac and Malet tried to transmit glanders by intratracheal injections and failed to produce the usual glanders lesions. They only succeeded in producing localised lesions—bronchitis and pneumonia, but no tubercles. Passing now to the digestive tract—to infection by way of the digestive tract—here Renault again was the first who conducted a long series of experiments by mixing horse foods with glanders material, sometimes succeeding and sometimes failing. Then there are the experiments of Cadéac and Malet alluded to, I believe, by Professor McFadyean in his paper. Cadéac and Malet employed various materials such as pieces of glandered tissue, nasal discharge, tubercles and pus, with which they fed their subjects, and in many cases the result was negative. They concluded that it was necessary, in order to infect through the digestive tract, to have a wound in the mucous membrane. Coming to a later period—so late as the summer of last year—we find that Nocard, who up to that time held the opinion that glanders was transmitted through the respiratory tract, had somewhat modified if not altogether changed that view. In April last he happened to have in his possession a mule which he kept for a period of four months under observation, testing and retesting it with mallein to satisfy himself that it was entirely free from the infection of glanders. He then commenced his experiments by feeding the animal with a potato culture of glanders bacilli enclosed in a sort of sandwich. After a certain time he succeeded in infecting the mule, and a post-mortem examination revealed lesions of glanders in the spleen, and, also I believe, in the lungs, but speaking from recollection, I am not quite certain of this. That is the first reported experiment conducted with cultivated glanders bacilli. Since then, he has experimented with two horses and an ass, using bacilli on each occasion, and every time succeeded in infecting the animals through the digestive tract. In May of last year he reported to the Central Veterinary Society of Paris the result of these three last experiments. The mule was killed, the ass died twelve days, one horse died nine days, and the other horse fifteen days after receiving the glanders bacilli. In concluding his report to the Central Society, M. Nocard makes the following remarks: "The experiments that I have made and that I am pursuing have fully convinced me of the danger of pails, mangers, and common drinking troughs." He also refers to Cadéac and Malet's experiments and he maintains that the bacilli can enter the system without an abrasion of the mucous membrane of the digestive tract. He is supported in this by Chauveau, who states as the result of his own experiments that glanders infection may occur without abrasion of the mucous membrane. The discussion was closed by Leblanc who remarked that the propagation of glanders through the digestive tract was well-known, and that when he was sanitary inspector for the district of the Seine he reported that one of the principal causes of the propagation of glanders amongst cab horses was due to watering out of the same buckets on the public roads." Apart from these experiments which, though not numerous, serve to show the possibility of infection through the digestive tract, there are those rather rougher experiments occasionally reported from Zoological Gardens where animals fed on raw horse flesh sometimes die of glanders. There are several cases on record, and I am very much inclined to urge that the digestive tract should not be lost sight of as rather a frequent pathway for the entrance of the organism or infective material of glanders. I noticed that Professor McFadyean referred to the absence of lesions in connection with the stomach intestine—the absence of lesions in the mesenteric glands—such as one would expect if the infection took place through the alimentary tract; but it is possible that the digestive fluids may have some kind of modifying influence upon the bacilli when taken with the food, and that influence may be equal to preventing local lesions. I throw out this hint which may suggest another view to some other member of this Society. If the bacilli pass from the digestive tract by the lymphatics, it is quite likely they will be arrested in their progress in the mesenteric glands, and if they are taken up by the blood stream then, of course, they ought to be arrested somewhere in the liver. But the question arises if the capillaries of the liver, if the vascular system of the liver is not on rather different principle from that of the lungs, so that the organism may pass through the liver without being arrested, whereas it cannot so easily pass through the lungs.

As to the distinction between the acute and chronic forms of glanders. I believe that this distinction was first made by Coleman. I quite agree with Professor McFadyean that acute glanders is not often seen now, but that is not the fault of glanders. It is because a great deal more attention is paid to nasal discharge—a great deal more suspicion is excited in the practitioner by nasal discharge, and 'doubtful' horses are not allowed

to live so long as formerly. I remember a time, not so many years ago, when acute glanders was rather common, I refer to a time when the glandered horse had a well-marked discharge from one or both nostrils and a submaxillary swelling, when the breathing was noisy or audible and horse in distress, when the temperature was 106, 107, and when the animal was killed the whole interior of the nasal chamber was found infiltrated and ulcerated, the lining of the trachea in a similar condition, and both lungs more or less affected with glanders lesions. That was the kind of case called 'acute'; it is not often seen now, but I remember the time when it was not at all uncommon.

I think Professor McFadyean is well aware of the many experiments that have been performed—some successfully—to ascertain the curability of glanders. It will not do harm to give some of the information and to recall the main facts to the professor's memory. One observer maintains that when the bacillus of glanders is passed through cats a mallein can be obtained from the blood and organs of the inoculated cat which can be used in the cure of glanders. For instance, one observer—his name, I think, is Bonome—has proved by experiment that by giving fourteen injections in forty days of the mallein obtained from a cat inoculated with glanders, he can get rid of all symptoms of the disease in a horse. By a similar process the same observer cured glanders in dogs but failed in the case of guinea-pigs. There is one authority on glanders whose name is very frequently quoted, I mean Semmer. Last year he contributed quite a long article on the subject of glanders, particularly concerning the curability of the disease, and he refers to the work of two Algerian veterinary surgeons, Decroix and Bougome. These gentlemen are said to have cured numerous cases of farcy by the cauterisation and extirpation of farcy ulcers, a practice that was very common in this country some years ago; and, so far as the external appearance, the general behaviour and well being of the animals afterwards were concerned the cases were apparently cured. Semmer also alludes to experiments in regard to the curability of glanders carried out at Turin by Brusaco and his pupils who are said to have cured 50 per cent. of glandered horses by treating them with carbolic acid, iodine, sublimate, and ferri sulph. Levi, and J. Neumann, claim to have cured horses with tracheal injections of iodine, and potash iodide. In France they have adopted a mode of treatment and succeeded to their own satisfaction by giving glandered horses creosote in oil hypodermically. Semmer, alluding to his own work, in conjunction with Itzkovitsch, states that they witne sed recovery in a notorious case of inoculated glanders and farcy, in a filly at Dorpat. Semmer further refers to the action of ox blood serum as being useful in the cure of glanders; and in this he is supported by another obsever called Barbes who maintains that ox blood serum has a beneficial therapeutic action in glanders. Then there is Pilarios who cured eight glandered horses in the first stage of the disease by repeated weekly injections of mallein. There is another case of cure—in the human subject—reported in *The Veterinarian* of last year. This case, too, was treated by frequent injections of mallein. Following similar lines Helman, whose name has been mentioned in connection with the discovery of mallein, has cured one case of glanders. The horse was subsequently tested with mallein and failed to react. Injections of blood serum from this horse applied to cats and guinea-pigs reduced the predisposition of these animals to glanders. I gather that Semmer prefers ox blood serum to mallein in the treatment of glanders. The serum has more effect on the bacilli, which when exposed to ox serum lose much of their virulence. To prevent misunderstanding I ought to say that a glandered horse is said to be cured when it ceases to react to mallein. So much for the curability of glanders. Referring to the symptoms I am glad to see that Prof. McFadyean has taken the trouble to notice the symptoms of glanders, because it is high time that the text books were revised with regard to the symptomology of glanders; and I must congratulate the professor (if it is possible for me to congratulate him), upon the symptoms which he lays down in the summary of his paper. With the diagnosis of glanders, as given in the summary, I agree, and consider that there are numbers of cases which are not diagnosable by any means short of the use of mallein. Horses that are healthy to all outward appearance, injected with mallein are found to react, and when killed their lungs show the characteristic lesions. With regard to mallein itself I have a few words to say. I want to know—if I can obtain the information—how mallein acts. That it is effective, that it is potent, that it has an extraordinary action no one who has used it can deny. It raises the temperature, it causes a swelling at the seat of injection, it makes some horses yawn, causes some of them to lose control of their limbs, to stagger, and to fall lame; it affects the appetite, and upsets the horse generally. If a small quantity of this material called mallein has such effect upon a horse in which post-mortem examination discovers a few tubercles in the lungs, we cannot withhold the belief that it must be a very powerful agent. Now, I want to know if mallein has a selective power. Has it a selective power for glanders lesions anywhere or only for those lesions in the lungs? If it has a selective power what action has it upon the lesion itself? Does it advance the lesion, does it alter it in any way, does it increase its size, change its consistency, or in what way does it act? Then I should like to know if tuberculine or any other organic fluid would have a similar action on a glandered horse. I have a recollection of some experiments performed with tuberculine, perhaps not as a diagnostic agent for glanders but in the treatment of glanders; and I believe, the result was that the glandered horse reacted to tuberculine fairly well. It is usually maintained that mallein is harmless on non-glandered horses. I do not wish to deny the statement, but I am not aware of any experiments conducted to prove that mallein is harmless to non-glandered horses. Assuming that mallein is harmless, it would be worth while to know if mallein has any protective power on a non-glandered animal, because if it can protect from glanders it would be better to malleinise a whole stud than to pick the horses out by degrees. If it were once established than a malleinised horse would resist glanders, then of course there would be a very great demand for the agent, and we should be really adopting a prophylactic medicine.

Next we come to suppression. I do not intend to enter into the question of legislation or to discuss what the government may or ought to do, what the London County Council should do but does not do. I wish simply to refer to what should be done, nay what ought to be done in a stable where a few glandered horses have been picked out by mallein or some other diagnostic means. In the light of recent research and of present knowledge regarding glanders, is disinfection of an infected stable essential or superfluous? That is the question. If we knew the radius of infection, the vitality of the virus, it might be found that disinfection is entirely superfluous; that once you remove the living animal no further harm can result in a stable until another glandered animal is introduced. It might be found that there was no occasion to destroy fodder, to burn litter, to whitewash, to scrub, to pick up, or to set about the damaging process of disinfection which is so popular. But we are in a predicament at the present time. We do not know what happens in an infected stable. Hence disinfection is practised and carried out and recommended, and certainly it is a good thing. It is a good thing

to disinfect on the same principle as that which led an old Scotch farmer to regard Sunday as a Godsend because on that day his servants got washed. (Laughter.) And I believe that this disinfection, even if it is unnecessary or superfluous in the after treatment of glanders is a good thing for the stable—particularly in London. Now on that point I would like Prof. McFadyean's opinion whether he thinks, from his own knowledge of the disease, it is necessary to pursue this elaborate disinfection of premises; and a further question arises from this, when does he consider that a stable is free from infection or fit for the reception of fresh, healthy stock. According to the discoverer Loeffler, the glanders bacillus dies a natural death after four months if left to itself, but according to Cadéac and Malet it is practically uninfective in a much shorter time. But my point is: when is the stable in a fit state for the safe introduction of a fresh stock of uninfected animals. I have given you a few questions, and I have probably suggested some points that may be taken up by other members of the Society; and if I have not re-opened the discussion to your satisfaction I have tried to do my duty. (Applause.)

Mr. J. A. W. Dollar said: Mr. President and Gentlemen, my appearance here this evening with a few books seems to have raised such apprehensions on the other side of the table, that I hasten to allay the honourable members' fears by at once disavowing any intention of speaking for long. One book I have here, however, happens to be specially appropriate on the present occasion because it enables me to correct the suggestion that Prof. Nocard did not recognise the possibility of infection through the digestive tract before the summer of last year. A statement to that effect has just been made by the learned professor on my right (Professor Macqueen), and I will, therefore, read you a short extract from the lectures on contagious diseases delivered by Professor Nocard during the year 1891-92, which will show you that even at that time the professor recognised the possibility of transmission of disease by ingestion. "This is precisely"—he says,—"the danger in country inns. A large number of animals pass through them. The diseased ones may leave the germs of contagion in mangers and buckets which serve for all strange animals Now, one knows that the horses of hawkers, canal bargemen and so on are often glandered, and as they only pass a single night—as a rule—at the same inn, one diseased animal may infect a large number of places or even a whole district." Then, again, he says:—"Although the ingestion of infected food may be the most ordinary method of contagion, it is rare that one finds a glanders lesion in the intestinal mucous membrane." That, I think, sufficiently disposes of the point that Professor Nocard did not recognise the possibility of infection by ingestion. Then a statement was made that Babès experimented on horses with the object of discovering whether it was possible to infect them by rubbing glanderous material into the skin, but that experiment, so far as I am able to ascertain, was not made on horses but on guinea pigs; and Professor Nocard alludes to it here [in a book produced] and suggests that infection really takes place through very small injuries in the skin or that after infection the animal either scratches the place or that it is wounded by pieces of straw or other material in its bed. These points, however, are quite beside the question to which I wish to address myself this evening, and which is a very important one, that, namely, of how infection usually takes place. Does it occur (as Professor McFadyean stated) through the respiratory tract or does it take place also and frequently through the digestive tract? In regard to this question it may be interesting to note what has been discovered in other diseases, and I should like to draw attention to the experiments of the Russian discoverer, Wyssokowitsch who found that the intact mucous membrane of the lungs resisted the entry of germs of typhoid fever, staphylococcus pyogenes and several other diseases, even when injected into the trachea. Arnold—an English discoverer—also addresses himself to this question. He found that particles of emery, ultramarine, or even of such finely divided material as smoke did not penetrate to the interior of the lung, although coarse particles were taken up by the mucous membrane of the alimentary tract. Therefore, I think, we have some ground for considering that the entry of organisms through the pulmonary mucous membrane directly into the substance of the lung is somewhat difficult. On the other hand Koch and Villemin tried certain experiments with tubercle bacilli which were more successful, but the conditions were of such a peculiar character that they could scarcely be compared with the processes normally occuring in cases of glanders. They took a number of white mice and subjected them for half-an-hour on three successive days to the vapour produced by spraying tubercle bacilli into a closed box. The animals in such cases succumbed, and it was discovered that the same results could be produced by directly injecting a solution containing the bacilli into the trachea. But a very singular fact came out during those experiments. Although it was found perfectly easy to produce tuberculosis, either by spraying this infective material into a box and causing animals to inhale it, or by direct injection into the trachea, yet dry powders did not impart disease. Now, I take it, if the disease (glanders) is usually spread by inhalation it is spread by dry powder, and therefore the experiments of Koch and Villemin would rather tend to contradict the suggestion that glanders is usually spread in that way. I think that this point really deserves somewhat careful consideration. We are usually told that an animal suffering from glanders distributes the infective material over the wall of the box and on the fittings around it, that the discharge gradually dries, then breaks up into an exceedingly fine powder which is carried by draughts of air to the uninfected animal, and is gradually drawn in along with the air inhaled. At the outset that theory seems to me to be met with a rather serious difficulty because Löffler, the discoverer of the disease, found that glanders bacilli died in eight days when exposed to the air on silk threads; and he found further that in no instance did they resist desiccation longer than three months. Renant, Pouch, Galtier and others went further, and showed that dessication at once destroyed the virus. Then, again, we must remember that the animal is provided with a very elaborate filtering apparatus, in the turbinated bones and upper air passages, which are excellently contrived for catching every particle of dust that enters the nose and which are almost sufficient to clear the air of everything except the very finest particles before it enters the lungs. I therefore think that we have to deal with two initial difficulties. At any rate, we have to explain how this moist discharge dries without the bacilli dying, and how the horny material so produced breaks up and becomes converted into fine powder,—a thing that seems to me exceedingly difficult to contrive even by design. Then it appears most peculiar that so large a quantity enters the lungs, and that if infection takes place through mucous membranes, the quantity which must necessarily lodge on the turbinated bones and in the upper air passages does not produce lesions there more frequently. Even when this discharge reaches the lungs there is a further difficulty which presents itself. We naturally enquire where is the powder most likely to be deposited; and to find cases tending to explain this point we may turn to diseases like "miner's lung" in which large quantities of fine coal dust are regularly inhaled, or to the cases of lead workers and wool sorters where a similar thing occurs. In these instances it is found that the whole lung is not affected but a particular portion only,

that, namely, where the air current is least pronounced: the point, in fact, where the circulation is almost *nil*. I may take an illustration from every-day observation. We all know that dust does not accumulate at a point in the street exposed to a strong wind, but if we wish to discover it, we must look in the eddies and blind corners. The borders of the lungs are the blind corners where the current is least, therefore, we would expect that these infective particles would be deposited at those points; and if the infection takes place directly, we should discover glanders there. But, I think, the essayist points out to us that it is one of the pecularities of the disease that the local lesions are not massed together but are generally distributed pretty uniformly throughout the diseased portions of lung. Professor McFadyean will correct me if I am misrepresenting him. Again, on the hypothesis that infection takes place through the mucous membrane of the lung, should we not expect that the mucous membrane would be the first portion affected. I think that, from the splendid series of photographs we have seen, it is pretty clear that the mucous membrane is not the first portion to be affected, and if I have drawn a wrong inference from looking at those photographs, I err in good company, because I could adduce the names of a considerable number of authorities in support of the view that glanders did not originate in the alveoli of the lung or mucous membrane, but originate—as a rule—in the deeper portions, and that the larger number of bacilli are not found in the mucous membrane at all; that, in point of fact, the disease advances by the lymphatics of the lung, and extends to the perilobular lymphatic spaces, whence it invades the lobule from the periphery towards the centre. The bacilli enter the peribronchial and perivascular spaces from which points they invade the vessels and bronchi. We also have a clinical factor to consider, namely, why is it that a period of incubation seems to be almost universally allowed by practical men to exist, during which the animal is incapable of transmitting the disease? I think that most practitioners are agreed that the animal which has been exposed to infection does not immediately become infectious in turn, but that a certain period intervenes in which it seems incapable of transmitting the disease to another animal. If the primary lesions were found in the mucous membrane, if they were on the surface, they would be in a position whence the bacilli could readily pass into the upper air passages and be distributed as discharge. We should expect to find that the animals would be infectious from the first day, or at any rate from a very early period after the growth and multiplication of the organisms. That is a practical point—a clinical point—and is, perhaps, worth notice. I may say, as rather tending to strengthen the position that the mucous membrane is impenetrable to micro-organisms, Von Besser has discovered that many other pathological organisms exist in the lung, such as streptococcus pyogenes, diplococcus pneumoniæ, Friedlander's bacillus and others, without in any way affecting the health of the animal. Buchner and Muskatbluth certainly show that bacilli passed through the mucous membrane and the lung, but in that case they would be anthrax bacilli, and anthrax is not, of course, a disease which we can fairly compare with glanders: it is not one in which we discover the same localised pathological conditions; and the case is scarcely a fair rebutting argument. Again, as tending to support the suggestion that glanders is most frequently passed from animal to animal by ingestion, we have the fact that animals fed on food infected with glanderous material show those small glanders centres which we all recognise in the lungs, within from 10 to 15 days. That statement is on the authority of Professor Nocard who made it at the congress at Buda-Pesth. This (showing pamphlet) is a small pamphlet containing the remarks he made on that occasion. The question then arises has any case been seen where the disease was due to inhalation and in which these glanders centres were produced in a shorter time. As to the course by which the bacilli may pass to the lungs, I have consulted various observers. Rabe for instance, says the tubercle (of glanders) originates in the depths of the parenchyma. That would tend to show that the infection was not directly from the mucous membrane of the lungs. Leclainche, professor I believe at Toulon, writing in 1893, considers that the lymphatics carry the infection and that the changes in the alveoli are always secondary.

Professor McFadyean: May I ask what you mean by disease of the parenchyma of the lung which proves non-infection by the mucous membrane?

Mr. Dollar: I believe that Rabe considers that the primary seat of the lesion is in the lymphatic vessels and that it spreads from there to peribronchial and perivascular spaces.

Professor McFadyean: Take no notice of my interruption. I admit it is not fair to ask you the question. It is not your fault. It is the fault of the gentleman quoted. He ought to have defined parenchyma.

Mr. Dollar (continuing): Leclainche considers that the lymphatics carry the infection, and he says that the changes begin at the point where the lymphatic vessels surround the blood vessels or bronchi. He further says that the changes in the alveolus itself are secondary. Nocard also thinks that the disease may be produced by ingestion, as is shown by reference to his book. In a small article which he has had reprinted, Schutz says:—"Glanders never seems to be found solely in the lungs, but is always present in other organs." Now that goes a long way, to my mind, in overcoming the difficulty every one meets with in taking up the subject, namely, why, if infection takes place through the intestines, are not the abdominal organs more frequently affected; why, in a word, should the lungs invariably appear to be the first seat of infection, and why should all the other abdominal organs be free from it? Schutz, in connection with Virchow, made a large number of experiments to test the value of mallein. In this article in my hand he gives the results of the post-mortem examinations. I believe that a hundred or more horses were experimented on, the results of the post-mortem examinations show that in a very large proportion indeed of these cases, there were lesions either in the liver, spleen, or other abdominal organs. Hence, purely from the clinical standpoint, I should rather oppose the view that inhalation is the principal source from which infection arises. There are the facts to be considered of the short life of the bacilli when dry, the difficulty one has in conceiving that the horny dried discharge can be broken up and converted into dust, the further difficulty in imagining why local lesions in the nose are so comparatively infrequent, and why changes in the lungs are so frequent, and also why the changes in the nose are usually so late in appearing. On the other hand, I should put the frequency with which horses lick infected walls, eat infected food and drink infected water. I think, therefore, we should not attach too much importance to the constant appearance of changes in the lungs, and I think something is to be said for the ingestion theory. I was going to make a few remarks on the question of immunity, but I find that Professor Macqueen has anticipated me. Straus made several experiments on a dog. He injected glanders bacilli into the blood stream and discovered that permanent immunity was given. On the other hand, he found in the dog no immunity appeared to be given by subcutaneous injection. I am glad to be able to supply the name of the observer who cultivated the bacilli for some generations in cats, and afterwards obtained immunity in a horse by injecting it. That gentleman was a Russian of the name of Lakharoff.—(Applause.)

Mr. W. HUNTING said we may roughly divide Prof. McFadyean's, paper into two parts, one of a clinical and sanitary character, and the second of a bacteriological and histological nature. On the two latter sections I should not venture to express any opinion. What Professor McFadyean tells me I accept with simple faith. With regard to the clinical part we differ in some respects, and I shall endeavour to give reasons for my difference on points where we fail to agree. Before doing so, I should like to refer to a question of Professor Macqueen's as to whether glanders is increasing or decreasing. I take it that nobody disputes that the disease is decreasing, and that the decrease is due chiefly to the work of the private veterinary surgeon and the spread of general intelligence. Even in my early days, (and my friend, Mr. Shaw, has, perhaps, a larger experience of the state of things that formerly existed than any man in this room), even, I say, in my early professional days of thirty years ago, there was at least ten times more glanders prevalent throughout the country than now, and perhaps three or four times more than at the present time in London. Until recently the returns on this subject were useless, and only showed the innocence of the individuals who made them. (Lghtr.) I think Mr. Sheather once drew attention to a remarkable thing about those returns, viz., that they showed that while farcy was increasing glanders was decreasing. That was not an unfair inference from the figures but it was not the correct solution. When notification of farcy was first required very few persons obeyed the law. Gradually more of the cases seen were notified and farcy, therefore, naturally greatly increased on the returns, and there came a time when there were as many cases of farcy as of glanders. I believe that in 1885 we had the lowest registered number of cases of glanders. In 1892 we had the highest registered number of glanders; and from 1892 until now, there has been a constant fall until the first few weeks of this year when there was a slight rise again. One more point referred to by Professor Macqueen. He asked when is a stable that has contained glandered horses free from disease? Well, of course, that depends mainly upon how much it happened to be infected to begin with, and how well or ill it was cleaned after removal of the horses. I have a little experience to give on the point. I once had a twenty-stall stable at a certain place cleaned out where "suspicious" horses were kept. It was in the days where the law and the authorities were not so particular. We removed from this stable the whole of the horses, we sluiced it down with cold water and left the mangers which held water full of water all night. The next day we washed down with soap and soda and the day after the walls of the stable were limewashed. The day after we filled up the stable with twenty new horses, each of which remained under my observation for three years during which time I never knew a case of glanders amongst them. I know another case where a yard—a large cab yard—was shut up for three months, after containing a stud from which an average of one horse per week was slaughtered as glandered. Another cab proprietor took it. It had, as I said, been shut up for three months during which absolutely nothing was done to it. The new proprietor had been in possession twelve months before he had a case of glanders, and that I believe introduced from outside. Now to refer to Professor McFadyean's paper. The professor says: "I do not know on what grounds it is contended that infection usually takes place by the alimentary canal." Now, by parity of reasoning, I may say that I do not know upon what grounds it is contended that infection usually takes place through the respiratory tract. There is experimental proof that it takes place from ingestion, there is no experimental proof that it takes place in any other way except by inoculation. The burden of proof lies entirely with those who think it spreads by inhalation; and, as far as I can make out, the only argument in favour of inhalation is the analogy drawn from the spread of tuberculosis and, perhaps, some other diseases. Not only has glanders been produced by ingestion but clinical observation points to the fact that nearly every case is traceable to ingestion. Let me be a little more explicit. There is a significant difference between the spread of glanders in a cab stable and in an omnibus stable. You have a different arrangement of horses in these two kinds of stables.

Professor MCFADYEAN (interposing): You say "nearly every case is traceable to ingestion," thus excluding the possibility of infection by respiration.

Mr. HUNTING: Yes; I am going to try and prove that. Cabmen, I may say, work two horses during their spell, one through the day and the other at night. The day and night horses do not stand next to each other. They are ranged in separate stables as a rule, so that the night may not disturb the day horses. The day and the night horses are connected only by a common nose bag, and yet we find that the disease spreads from night horse to day horse—to those in fact in which ingestion seems to be the only method of infection. On the other hand, in the omnibus stable you have a stud of ten horses in a row and you will find, as a rule, that the horse first infected is followed by the one standing next to it or at any rate in the same stud. Here the connection is the manger and direct infection of food by sneezing. In many stables there is a water trough interposed between each manger. This is absolutely never dry, nor is the food in the mangers. It is a perfectly well-known fact that even with iron mangers, or undivided mangers, that what with the saliva of the horse and the slopping over of water from these troughs, the manger is never dry; yet the disease spreads there from, I say, the direct contamination of the food and its being swallowed by a horse. I may give an illustration in disproof of the inhalation theory, an instance in which that theory cannot be applied. I found a horse suffering from glanders in a square stable—a stable in which the horses stood on all four sides. Some remark was made, and the horsekeeper told me that the diseased horse had only stood in its new position that day. He added that he stood "over there"—indicating another stall—"yesterday." I asked was he in any other stall. The man replied "yes, he was in that stall the day before,"—indicating another place. I asked "Why in the world were all these changes." The man replied that the horse being a grey his employer told him to put the animal among a stud of greys, and let a chestnut take its place. Next day the grey was found to be ill and was moved to a stall under a window. The following day it was moved again to a warmer stall and when I saw it, it was back under the window. I took the precaution of going over the stable and taking the number from the horse's foot that occupied each of these stalls that the diseased horse had been in; and within three or four months I had to pole-axe every horse that had followed the glandered horse into the different stalls. It seems to me impossible upon any theory of inhalation to account for the passage of the disease from that one horse to the other three horses. Now, just as Mr. Dollar said, let me repeat again the difficulties which surround the spread by inhalation. The bacillus must be dried, and as Professor McFadyean told us in his address it is very easily killed. I do not know whether anyone has tried the experiment of thoroughly drying the bacillus to see whether it survives or not, but I do not think it survives long anywhere outside an animal body. We know that strong sunlight kills it very rapidly, and we know also that in moist places you have the putrefactive organisms which kill it even more quickly. Glanders bacilli have a very bad chance of

surviving or of becoming dry dust if left about a stable seven or eight days, and it will probably require fourteen days before they get dried in such an impalpable form as to float about the atmosphere of a stable. To my mind it is a far fetched theory to account for the spread of glanders on the hypothesis of dried bacilli passing into the lungs with the air after escaping all the dangers lying in wait for them. It seems simpler and more direct to accept the explanation that infection takes place through the alimentary tract by the living bacillus passing direct from a diseased horse into the food or water of another. As to tuberculosis I am not inclined to allow that the analogy holds at all. I do not know that tuberculosis is nearly always the result of inhalation. I have no right to say that it is not but I do not believe it is, and I want some proof of it. But even if this be so, it is a different disease to glanders. To begin with, you argue that because you find lesions very constant in the lung and very seldom in the abdominal viscera that the lung is the primary seat of infection. You say that in tuberculosis there are always lesions at the point of entrance of the bacillus through the bowels or skin. I say in glanders there are not (Professor McFadyean : I did not say that.) When I say "you" I mean everybody who takes that side of the question Now, in glanders it is different. You have more or less permanent lesions left as the result of the blocking in a part of the tubercle bacilli. If you have a gland once affected with tubercle it is more or less permanently affected. In glanders this is not so. Even the large gland under the jaw may be double its normal size and soon resume its normal condition, and twelve months afterwards you cannot find a single trace of local lesion. And the same with the bowels. It is possible that if you were to make a post-mortem examination very shortly after infection by food you might find some lesion in bowel or mesentery, but I have never found those lesions. Schutz says he has, and I am not inclined to allow that Schutz is right because he is a foreigner. I want to find a man in England who has seen bowel or mesenteric lesions. Until I find them I shall doubt their existence, and believe that the distinguished bacteriologist found something which he recognised as glanders but which was not glanders. If there are glanders lesions in the skin such as farcy buds which clear up, so may lesions through the bowels clear up. If we once acknowledge—as we do acknowledge—that with water and food, glanders may enter the body and having entered locate itself in the lung, why should we seek for another explanation of infection because another disease, not exactly analogous, is said to infect through the air passages? Passing away from this question, I chiefly want to touch on the means of suppression, but there are two or three points which I should first like shortly to notice. As to the vitality of the bacillus, I know very little, but I certainly believe that when it fails to reach the lungs it does not survive very long even in the horse. I say that because I should expect if the bacillus floated about in the blood or lymph stream we should more constantly get local lesion. We know that a horse may have glanders bacilli in his lungs for years and years without an outward sign of disease. I believe, therefore, that unless the glanders bacilli gets comfortably lodged in the lungs it perishes rapidly. The majority of cases of glanders are non-infective until some external symptoms develope. It might almost be regarded as a local disease of the lung—a local disease, however, which may at any time become infective or general by the bacilli escaping and passing from the lungs by the blood and lymph vessels over the whole body. As to the incubative period, I have never experimentally tried it, but the shortest period I have yet come across is one month from the time of infection, and in that case the horse had lesions in the lungs Now between the incubative period of the disease and the latent period. I have before tried to draw a distinction—and there ought to be a distinction. The incubative period, I take it, is the time from when the bacillus enters the body to the time when you find lesions anywhere in the body. The latent period I take to be simply the period in which an animal may be infected and yet show no external symptoms of infection. I suppose that Professor McFadyean will tell us that there is no latent period now that we have got mallein to indicate the existence of lesions. With regard to curability, Professor McFadyean answers the question "do cases of glanders ever end in recovery?" in the affirmative. And Professor Macqueen gives us some particular cases of recovery. Personally, I think I know two or three undoubted cases. As to farcy—if I may distinguish between the two forms—I know some dozens of cases of recovery. I am not going to say that there is any particular practical value to be attached to the fact that occasionally horses recover from glanders. The fact has one important bearing on stamping out the disease for the public good. If you allow that glanders is curable then you have one of the strongest arguments in favour of fair compensation during the process of stamping out, otherwise you would be killing a horse every now and then that is curable; and you have no right to do anything of the kind without paying compensation. With regard to "susceptibility," I do not believe a bit in the susceptibility of horses to glanders. I believe that the strongest and healthiest horse if he only gets the bacillus among his food (I don't know about the dry one in his air supply) will succumb to glanders, but it will be longer before he does succumb. I believe that the reason why glanders spreads more rapidly in stables where the horses are underfed, overworked and badly treated is simply this: You have under those conditions a much more rapid development of the disease—it more speedily reaches the stage at which it is most infective. In other words, as horsemen say, they "break up" more quickly. When you have more acute cases or more generalised disease the discharges from the body are more infectious, and thus you have the disease spread very rapidly. With respect to diagnosis I will only say that I believe with Professor McFadyean that we have in mallein not only the best aid to diagnosis, but an instrument of diagnosis which is much more certain than any human being can possibly be. Mallein makes very few mistakes. It diagnoses cases which the cleverest and best observer cannot by any possibility diagnose. In 178 cases in which I have used mallein I have only seen three cases in which I failed to find any glanders lesions on the post-mortem examination. It may be in regard to the latter that I do not recognise the lesions, that I was unable to find them, or that they did not exist. I only state the fact that we could find nothing. I have never known mallein fail in detecting glanders when glanders was existent, so far as cases go in which I have seen the post-mortem examination. In cases where the temperature was very high, and in which mallein causes little rise, I have never known a single instance in which there was not some external clinical signs to help you. It is very curious to note how accounts are published in this country from foreign authorities. I notice that Professor Brown has quoted some gentleman on the Continent of Europe, in an article he has just written for the Royal Agricultural Society's journal. After quoting authorities and summing up the advantages and disadvantages of mallein, Professor Brown mentions four "conclusions." No. 4 is "In an acute case the local and general reaction was most intense, but the rise of temperature was not very marked." In this country we find no rise in acute cases where the temperature is already 104° or more, and the general reaction is not intense.

As to means of suppression, mallein has, as Professor McFadyean states, removed our greatest difficulty; but I can not allow that it is so indispensible as he says. The Professor says: "Does anyone suppose that in studs of hundreds of horses in which the disease has prevailed for years, the disease can be exterminated by isolation or slaughter of animals chronically glandered?" Prof. McFadyean overlooks the fact that we have stamped out glanders in studs of hundreds of horses without mallein. In the British Army there are 16,000 horses, and glanders was stamped out among them some years ago without the aid of mallein. I do not say that to lower the claims of mallein; but I wish to point out that we were not absolutely useless before mallein was discovered. I believe myself that one of the great values of mallein for some time to come will be its use by the private practitioner because authorities, local or central—the Board of Agriculture for instance—are slow to accept anything new or to do anything—well, they are slow. (Laughter.) Individual effort has now a chance that it never had before. A man got tired of his veterinary surgeon and of killing this and that animal whilst year after year the disease decreased but remained. Now, if a veterinary surgeon gets hold of a sensible man—there are many of the other sort, (laughter)—he will allow him to go through his stock with mallein. If he allows him to do this for a week or two, or a month or two, he will thus stop the spread of disease in a stud as you could not in any other way. Now, before expressing any views as to what I think should be the way in which the disease should be met, I want to take exception to two or three paragraphs of the essayist. He says: "If in any stud, however large, glanders continues to exist, the owner will have himself to blame." I am not going to dispute that as an abstract proposition, but I am going to object to it as a practical fair statement. I am going to state that, under the circumstances, it is not right to impute blame to the horse-owner in London for having glanders in his stud. Everybody knows the extreme difficulty of getting rid of the disease, everybody knows how easy it is to have it re introduced into a stud when once it is got rid of. Some of the cleanest and best kept stables in London have suffered from glanders; and I unhesitatingly assert that there is no large stud in London that has ever remained wholly free from glanders for ten years. I myself have seen certain studs which have been reported to be free from glanders and I know that the report was untrue.

Professor McFadyean: Not free from glanders for ten years?

Mr. Hunting: Not for two, some of them. I know of no large stud clear for ten years. It is a man's misfortune to have glanders in his stud, not his fault.

Professor McFadyean: To get it.

Mr. Hunting: It is his misfortune to get it and his misfortune to keep it. We will take an illustration. Suppose a widow left with 40 horses and a carrier's business. Suppose everything squared up and only a business connection and an infected stud left. You suggest that mallein should be used in her stable, and that if fifteen out of the forty horses react to mallein they are to be killed. That would mean bankruptcy direct—absolute bankruptcy.

Professor McFadyean: I don't suggest it.

Mr. Hunting: But I suggest it as being the logical outcome of your statement. She could probably avoid bankruptcy if she killed one or two of the horses occasionally. In that way it would take longer to stamp out the disease, but she would save her credit and not become bankrupt. The other plan means having a long purse indeed; it means such an interference with business as very few firms in London could stand. To kill 20 horses out of a stud of 100 horses means upsetting that business, or the immediate purchase of 20 fresh horses and you cannot buy 20 fresh horses whenever you like. I quite agree that if a man is in a position to stamp out all disease at one time that would be the best thing in the end.

Professor McFadyean: I never advocated stamping out by slaughtering all the horses in a stable.

Mr. Hunting: So far we agree. I believe very much myself in mallein as a valuable auxiliary. I believe, as Professor McFadyean says, that the great difficulty of stamping out of glanders has been that, without mallein, there were cases which could not be recognised, and so long as a case is unrecognised it is extremely risky to wait for the development of symptoms. There are many cases where a glandered horse remains in the stable for months without infecting another horse. It is not at all uncommon to have five or six glandered horses in a hundred stall stable and then no more. Professor McFadyean suggests that there should be a law prohibiting anyone from selling a horse from an infected stable unless all the horses had first been tested with mallein. The effect of that would be this: when you had cornered one or two men in that way, no owner would report a case of glanders. An owner would say "I am not going to be had again like that, have my business interfered with and every horse tested with mallein simply because I reported. If I do not report, probably the existence of the disease in my stable will not be known; and, if the worst comes to the worst, I had better pay a fine of £20 for keeping my secret, than have my business upset." I have come across a strong supporter in this belief in the writer of a valuable article for farmers which appeared in the Royal Agriculture Society's Journal—I mean Professor Brown. Speaking of mallein he says: "Owners of horses will not be disposed to give notice of disease,still less to submit animals to mallein, unless they are sure of compensation." I agree with Professor Brown. It would pay horse-owners far better to test the whole of their horses with mallein, and sell the dubious ones to their neighbours after the swelling had subsided. (Laughter.) So long as the law does not prevent them doing that, and so long as no fair compensation is given, I fancy that individuals will use mallein to get rid of glanders, adopting the quicker, cheaper, and safer process of clearing himself by waiting until the swelling goes down, and then selling the infected animals. Would it not be better for the authority—local or central—to make some such voluntary arrangement with him as this: to say to him "knowing you have a case of glanders, if you will allow us to go through the whole of your stud and see which are infected, we will accept the reaction of mallein as indicative, and knock on the head every horse we find to be glandered. If we should make a mistake we will pay the full value for the animals not glandered, while for those which are glandered we will pay one-fourth the value." In that way you would induce men to report, in that way you would have perfect control and perfect command of the stable and not at a very great cost in the end. Of course such a plan would entail a heavy expenditure for two years, but would be found an economical as well as a speedy and an effective remedy for the existing evil. (Applause.)

Mr. Singleton: Considering the lateness of the hour and in view of a long journey homewards, I beg to propose that this discussion be adjourned to our next meeting; and I do so in perfect cognizance of the unfortunate conditions imposed upon one who moves such a resolution.

Mr. Butters: I have great pleasure in seconding the motion.

The Chairman: May I ask Professor McFadyean whether he can make it convenient to come to our next meeting on May 2nd.

Professor McFadyean: Unless something quite un-

forseen transpires to prevent me, I shall certainly come; I may say that I should come even to a continued discussion with pleasure. (Applause.)

THE PROPOSED VETERINARY COLLEGE IN DUBLIN, AND ITS RELATIONS WITH THE ROYAL COLLEGE OF VETERINARY SURGEONS.

THE EDITOR IRISH DAILY INDEPENDENT.

SIR,

As it seems probable that some definite action will shortly be taken with reference to the establishment of an Irish Veterinary College, it is well that the public should know something of the manner in which the veterinary profession in this country is treated by the Royal College of Veterinary Surgeons.

This corporate body is the only one which can examine students and grant diplomas in Great Britain, so that if the Irish College is founded its students must be examined by this body. This being so, and also the fact that a large proportion of the students attending the Scotch schools are Irish, would make anyone imagine that some of the Examiners of the Royal College of Veterinary Surgeons should be selected from among the Irish practitioners. But what do we find at the election of examiners that took place in London about two weeks ago? Although there were three applicants from Ireland, one of whom had been on the Examining Board for several years, yet not one of these were elected, and men were appointed whose claims over the Irish applicants we fail to discover. All the Irish applicants were thoroughly qualified for the posts that they were nominated for, and the only reason we can suggest for their rejection is the very old one of "No Irish need apply."

So that the Examining Board for Great Britain of the Royal College of Veterinary Surgeons, consisting of 22 members, does not contain a single Irish veterinary surgeon. And looking at the Council of this College what do we find? Why, there is only one Irish practitioner amongst the large number of members of Council. This state of affairs is simply monstrous, and if the Irish practitioners are not fairly represented both on Council and examining board it will be necessary for them to take steps to assert their rights.

At the meeting of the Royal College of Veterinary Surgeons in June, 1894, a motion was brought forward, instigated by the teaching schools, calling on the Government to rescind the proposed grant to the Irish College.

Fortunately this was defeated by a small majority, but what did it mean? Simply that if the Irish College was founded the owner of the Scotch colleges would lose money.

We fear that there is very little philanthropy in the matter; it is all a question of the students' fees. If the Irish College is founded, we sincerely hope that it will aim at giving practical instruction to the students. At present we must admit that clinical work, in cattle especially, is very much neglected at colleges, and the time spent in the lecture room, listening to a lecturer demonstrating on diagrams, would be far better spent in the cow shed among sick cattle. At present the standard of examination required under the new rules is far beyond the resources of many of the schools, so far as practical instruction is concerned.

But the Council imagine that by appointing medical men as examiners in pathology the standard of examination will be raised. There are plenty of men in the veterinary profession who are thoroughly well qualified to act as examiners in this subject. Surely, veterinary surgeons are far better able to judge of the necessary amount of pathological knowledge required by a student than any number of medical men, who know nothing of the requirements of his profession.

What the student of the present day requires is more clinical instruction and practical acquaintance with disease in its many forms, and less of the germ theories and laboratory studies. It is the clinical instruction that will enable him to be a successful practitioner, and to earn his living.—Yours, &c.,

AN IRISH PRACTITIONER.

THE BOARD OF AGRICULTURE.

The Daily Chronicle of April 18th has the following paragraph on this subject.

"The veterinary surgeons are up in arms about the Board of Agriculture and the way in which it is paltering with swine fever, and allowing non qualified inspectors, that is, men who have not had a veterinary training, to undertake 'such purely technical work as the investigation of pathological problems.' It is roundly declared that the extension of the duties of the lay inspectors 'to work for which they had not been specially trained' and the removal of the veterinary surgeons 'from work to which they were suited' is one cause of the extremely slow progress made in arresting the spread of swine fever, and is a departure from the older and sounder practice under which more than one contagious disease was stamped out. Now the stamping out of pleuro-pneumonia was also left to the travelling inspector, but 'with the aid of the veterinary surgeon of the district.' Surely the rule for one disease should be the rule for the other?

OBITUARY.

THOMAS GREAVES, Knott Mill, Manchester.

Graduated June, 1841, London.

On the 17th inst., at 3, Greenhill Street, Greenheys, Thomas Greaves, Esq., F.R.C.V.S., (of the firm of Greaves, Faulkner, and Jones, Knott Mill, Manchester), in his 78th year. Also, on the same day, Susanna, his wife, in her 81st year. Will be interred at Bowdon Parish Church, on Saturday, the 20th instant, at twelve o'clock.—*The Manchester Guardian.*

GEORGE A. DREWE, Abingdon,

Graduated May, 1857, London.

A painful sensation was caused in Abingdon on the 10th inst., when it became known that Mr. George Augustus Drewe, M.R.C.V.S., had committed suicide at his residence in the Vineyard by taking poison. He was engaged in business in the town as usual during the morning, and had stated his intention of going to London. He returned home for dinner and shortly afterwards was found lying insensible upon the sitting-room floor; beside him was an ounce bottle which had contained prussic acid. Mr. Drewe, who was 58 years of age, and came from a Berkshire family, has practised for many years in the district as a veterinary surgeon, where he had a large connection. He succeeded his father as the proprietor of the Junction Hotel, Didcot, and also for a short time as lessee of the Queen's Hotel, Abingdon, under the late company. As hon. secretary of the Abingdon Agricultural Society, Mr. Drewe rendered valuable service for a long period, and his retirement from the office some years ago was received with general regret. He took an active interest in cricket and all sport, and was starter at the Berks and Oxon Hunt Steeplechases. He was a director of the Abingdon Gas Company, and had held a seat in the Town Council for twelve years, having been elected by a large majority in April, 1883, and was re-elected the last time in November 1893. Much sympathy is felt for the widow and large family of deceased in their bereavement. At the inquiry held at the residence of deceased on Thursday morning, before Mr. E. M. Challenor, deputy coroner, the jury returned a verdict of "suicide while of unsound mind," and endorsed an expression by the deputy coroner of sympathy with Mrs. Drewe and the family in their grief. They also decided to purchase a wreath with their fees.—*The Abingdon Herald.*